simple to Stokes and Thomson in 1852, and to Stewart and Kirchhoff a few years later.

I wish to consider briefly, what are these new and puzzling complications of the solar problem; and whether we may not still preserve our belief in the existence of *essentially different* elementary atoms, which is the basis of the beautiful Vortex Theory. For it seems that to hazard (however *naturally*) such a step as is involved in assumed dissociation of the (so-called) elements, before we make certain that no less serious hypothesis will account for the observed facts, is contrary to the spirit of Newton's *Regulæ Philosophandi*.

The most prominent of these complications seem to be—

(1). The variations of the relative brightness, width, &c., of the lines in the spectrum of a particular substance, in dependence on the source and circumstances of its incandescence.

(2). The so-called "long" and "short" lines. (These, as will be seen, are probably a case of (1).)

(3). The fact that, in the spectra of sun-spots, *some* lines supposed to be due to a particular element indicate rapid motion of the glowing gas; while others, supposed due to the same element, give no such indication.

(4). The (at least apparent) coincidence of lines in the spectra of two or more elementary substances.

To these may be added:—

(5). The remarkable peculiarities of star-spectra; especially the paucity, and the breadth, of the lines in the spectra of *white* stars.

As regards (1), let us consider a sounding body with a large number of different modes of vibration, exposed to impacts either periodic or at least with an average period. The relative intensities of the various notes which it can give will obviously depend upon the period of the impacts. Now this is precisely the case of a particle (I use the word to avoid misconception) of a glowing gas. The average number of blows it receives will depend on (*a*) the number of particles per cubic inch (and also upon *whether there be another gas present or no*, a point of very great importance) and (*b*) the temperature, which is directly connected with the velocity of the particles.

Change the density, the temperature, the admixture with foreign substances, or any two, or all, of these; and the *average period of the battering* to which a particle is subjected may be so altered as to elicit from it *in any required ratios of relative intensity* the various simple rays it can give out.

It will readily be seen that this may account for all of the phenomena of classes (1) and (2) above.

(3) may be accounted for in many ways. I mention only one, as my object is merely to show that we are *not yet* compelled to accept dissociation of so-called elements even in its mildest form. Other modes of escape, though not quite so simple, present themselves.

What is seen in a sun-spot is the integral, as it were, of all that is taking place (as regards both radiation and absorption) in many thousand miles of solar atmosphere, containing the same substance under the most varied conditions. That portions in which certain lines of that substance are prominent over others may be at rest relatively to the observer along the line of sight; while others, in which (from different density, temperature, or admixture, as above explained) other lines are specially prominent, may have large relative velocities, is certain. This would at once account for these singular observations.

As to (5) we must remember that in a star spectrum we have, as it were, a *triple* integral. For we not only integrate through the depth of the atmosphere, but also over the whole surface of the star; spots, hurricanes, and rotation of the whole, included. This is equivalent to the *superposition* of innumerable separate spectra, no two of which may have *any one* individual line in the same place or of the same breadth, &c. Feeble lines may, in fact, entirely disappear under such treatment.

(4) If not due to want of dispersive power in the apparatus, this may be legitimately attributed to inevitable impurities. It is only in "tall talk" (or in advertisements) that any human preparation, elementary or not, can be spoken of as "*chemisch rein*." And we all know how faint a trace of impurity can be detected by the help of the spectroscope.

Even in the last resort, I see nothing to hinder the existence of exactly equal vibration-periods in two perfectly distinct vortex-atoms:—though their occurrence is extremely improbable.

If we could get an absolutely transparent gas; one, therefore, which could give no radiation under any circumstances; the study of the behaviour of a given quantity of hydrogen mixed with different proportions of it in a vessel of given size, and subjected always to the same conditions of incandescence, would give us invaluable information. G. H.

Replacing Flakes on Palæolithic Implements

THIS wonderful feat was first performed by my friend Mr. F. C. J. Spurrell of Dartford. On first thoughts the thing seems utterly impossible, and it is obvious that no flake can possibly be replaced upon an implement unless one lights on the exact spot where the instrument was made, and finds both implement and flakes in position. Mr. Spurrell so found his material. During the present summer I have discovered another and similar Palæolithic floor, far removed from Mr. Spurrell's, and where implements and flakes are exposed in a stratum perfectly undisturbed since they were gently covered up in Palæolithic times with fine sand containing the shells of such freshwater molluscs as Unio, Cyrenia, and Bythinia. For obvious reasons—the chief one of which is that my work would be totally stopped if I mentioned the locality—I will content myself with stating that the position is nearly a mile from any river, and the *floor* is 41 feet above the level of the nearest stream; above the floor is a thick deposit of fine stratified sand, and above that loam. On this Palæolithic floor I have found several implements and a large number of flakes, and on one of the finest implements, an example 6 inches long, $3\frac{1}{8}$ inches wide, and weighing $1\frac{1}{4}$ lb. I have been able to replace two flakes, one $2\frac{1}{4}$ inches long, the other 2 inches in exact position; the flakes slightly overlap each other on the implement, and both have been struck from the edge of the implement at right angles across its face. The implement and flakes were close together, and with them I found a hammer-stone of flint with a distinctly battered and abraded edge. Mr. Spurrell replaced many flakes round his implement, but the implement itself was a spoilt and poor example. My implement, on the contrary, is an unusually fine one, large, heavy, and perfect. Both the implement and flakes show a little of the original grey crust of the flint from which the instrument was made, and this peculiar grey colour led me to attempt the replacement of the flakes with the above-mentioned successful result. One flake has a slightly uneven edge—in some instances considered a proof of use—the second flake is quite sharp. I shall exhibit this implement, with other implements, flakes, &c., from the same place, at an early meeting of the Anthropological Institute. WORTHINGTON G. SMITH

125, Grosvenor Road, Highbury, N.

Integrating Anemometer

PERHAPS the following brief description of the integrator devised by me will suffice to establish its near kinship with Mr. Wilson's (NATURE, vol. xxiv. pp. 467 and 557):—A roller with a spherical edge is made to revolve with a velocity proportional to that of the wind as recorded on an anemogram. This roller presses on a plane table carried by two mutually perpendicular pairs of rails in planes parallel to that of the table. The lowest of the pairs of rails is supported by a frame carried on the extremity of a vertical shaft. The point of contact of the roller with the table lies in the prolongation of the axis of the shaft. The table can rotate with the shaft, but not independently. By a simple arrangement the shaft, and consequently the table, are caused to take up positions corresponding from moment to moment with the direction of the wind record on the anemogram. A style concentric with the shaft presses lightly against a compound sheet of tracing and carbonised paper attached to the under side of the table. Arrangements are also made for obtaining the sum of the movements of the table toward each of the four cardinal points. If the roller be moved with a velocity proportional to that of the wind, whether directly by a cup-anemometer or by a mechanical translation of the trace as given by such an instrument, while the table simultaneously assumes orientations corresponding to the direction of movement of the air, the line drawn by the style will be a miniature copy of the path of an imaginary particle animated by the movements actually belonging to the masses of air which successively affect the anemometer at the given station during the selected period, rigorously in accordance with the principle known as Lambert's. But in order that the trace drawn as described should correctly represent the actual movements of the air, it is evident that the whole mass of the atmosphere must be supposed to move "parallel to itself," *i.e.* in such a manner that the straight

line joining any two particles of air shall always be parallel to its original direction, an assumption which is manifestly incorrect. If I rightly understand the description of Mr. Wilson's integrator on p. 467, the trace given by it is precisely that which has just been shown in the case of my own machine to be based on a fallacious assumption. But though the *trace* may be useless, the summation of the movements of the table above described gives results which are representative of physical realities, being in fact the quadrantal components of the wind-movement at the station during the period dealt with by the machine. I trust that the preceding remarks will suffice to justify the statements contained in my last letter. Dr. von Oettingen's remark, referred to in my concluding sentence, related, not to his wind-component-integrator, but to the continual change of form in what may be called the *physical* Lambert's line, and implied the consequent advisability of discarding Lambert's method of treatment.

CHARLES E. BURTON

38, Barclay Road, Walham Green, S.W., October 14

P.S.—On September 21 last I forwarded to Prof. Stokes a description, with drawings, of two forms of wind-component integrator, suitable either for attachment to a cup and vane-anemometer, or for the reduction of existing anemograms of the pattern adopted by the Meteorological Office; and of simpler mechanism than my earlier machine, or Dr. von Oettingen's.

Calabar Bean as a Preservative

As many find such a difficulty in preserving entomological and other natural history specimens it may not be uninteresting to your readers to have a brief note on the use of Calabar Bean as a preservative. About eight years ago, when Aquilla Smith, M.D., Professor of Materia Medica, Trinity College, Dublin, was showing me through the museum that he has rendered so famous, I was struck by the perfect manner in which the specimens were preserved; the little brown beetle that is generally such a pest in similar collections being entirely absent. Dr. Smith told me that he treated the specimens with tincture of Calabar Bean, and very kindly gave me a bottle of the tincture. I used the tincture freely in my cabinet of Lepidoptera, and, although the collection has been wofully neglected since, it has remained quite free from mites. Dr. Smith tells me that the tincture was prepared by Mr. Squire of 277, Oxford Street, London, its strength being one part of the bean to eight of (rectified?) spirit. I might mention that Mr. Fetherstonhaugh used some of the tincture which I gave him in his cabinet, and was delighted with its action. A drop of the tincture is placed on the body of the insect. I found it a good plan to do this whilst the insect was on the drying board, as otherwise, in newly set insects, the damping with spirit caused the wings to spring.

E. MACDOWEL COSGRAVE

A Correction

I FIND that the term "glissette" is not used precisely in the sense which I had supposed. A reference to Mr. Besant's "Notes on Roulettes and Glissettes" (which I had not before me last week) shows that the envelopes of the moving lines, to which the theorem in my last letter refers, would be properly described as *roulettes*. It is obvious, however, that glissettes are in general also roulettes.

GEORGE M. MINCHIN

Royal Indian Engineering College.

Effect of Green in Painted Windows

I NOTICED to-day a curious effect in the east windows of Old Upton Church which may interest artists among your readers, and of which I should be glad to see any explanation. The pattern is in small regular pieces in which a strong red is prevalent, especially in the ribbon round the edge. Green is perhaps the least represented in area. At all events, generally, red largely prevails over green. The latter is not over brilliant. At a distance of ten feet the general effect is red. At that distance I see the pattern sharply, and green is not at all obtrusive. At the length of the church, say fifty feet off, I cannot distinguish the pattern, and the whole window looks a thin watery green haze; the bright red margin is inappreciable.

Richmond, October 12 W. J. HERSCHEL

THE AUTUMN MEETING OF THE IRON AND STEEL INSTITUTE

AT the meeting of the above Institution, which has just taken place, several papers of scientific and practical interest were read and discussed. They may be broadly divided into two classes, viz. 1st, those relating to the production of iron and steel, from the ore, and the qualities of the material when produced; and 2nd, the various applications to which steel has been put in recent times. The latter class of papers, at the recent meeting, dealt principally with the use of steel in the manufacture of ordnance, small arms, projectiles, and gun-carriages, and the papers, some of which were of great interest, will be reserved for consideration in a separate notice. Amongst the papers dealing with the manufacture of steel we may notice specially a memoir by Herr Paul Kupelweiser of Witkowitz, in Austria, on recent progress attained in the use of the basic process at the works with which he is connected. This process, which has been frequently referred to in NATURE, seems—probably on account of the quality of the ores met with—to have been adopted more frequently in Continental steel works than in our own country, for according to Herr Kupelweiser's summary, no less than thirty works in France, Belgium, Germany, Austria, and Russia, have acquired licences under the Thomas patents, the greater number of these being already at work; while the remainder are adapting their old plant, or erecting new works with the view to its immediate introduction. The weak point of the process hitherto has undoubtedly been the want of durability in the refractory linings of the converters, and on this point the author states that, in spite of numerous trials with other materials, the works with which he is acquainted still use the materials originally proposed by Mr. Thomas, viz. either the basic bricks or the shrunk lime and tar mixture. At Witkowitz, however, a new material has been used containing a comparatively small percentage of silica, and the quality of the bricks manufactured from this has been found to be materially improved. Ground brick mixed with 5 to 10 per cent. of tar is also used at many works for lining as well as for repairs. Basic tuyères have been tried in many places, but are not commonly used; but the author states that magnesia obtained by precipitation from chloride of magnesia by milk of lime appears, from experiments made on a small scale, to be a promising material for making tuyères. As regards the quality of the steel he makes the following remarkable statement:—"The basic process, as regards the quality of its products, is not only completely equal to the acid process, but even, in my opinion, superior to the latter." As a specimen of the excellent quality of the mild steel manufactured at Witkowitz the author exhibited a locomotive boiler tube made of this material, which had been expanded cold by means of a tube expander from 9 to 17 millimetres, on an original diameter of 48 millimetres, equal to an extension of from 20 to 36 per cent. on the periphery of the material, without even splitting at the line of weld.

Another paper of great interest to foreign manufacturers was Prof. Tünner's memoir "On the Use of Lignite or Brown Coal in the Blast Furnace." It is well known that the Austro-Hungarian Empire contains immense deposits of this fuel. It would be difficult to over-estimate the benefit which would accrue to the iron industry of Austria if this abundant and inexpensive fuel could be used successfully in the blast-furnace. All the experiments made in this direction till last year were of a more or less isolated and unsatisfactory character. In June, 1880, however, the "Mining and Metallurgical Association of Styria and Carinthia" appointed a committee to investigate the subject afresh. This committee has not yet reported, or indeed concluded its labours, but it is satisfactory to learn that it is fully acknowledged that there is no theoretical difficulty in the way of smelting